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REMARKS ON THE STATUS OF SPECIES AND SUBSPECIES.

BY H. A. PILSBRY.

In several articles published in recent issues of this journal, a conchologist as well known for his wide experience in the field as for the vigor and point of his fluent pen, has been at some pains to criticise certain work by the writer, on American land shells. Several newly named varieties of well known shells are particularly obnoxious to my good friend; and in another place¹ he attacks the recent systems of classification of land shells, darkly hinting at certain tabulated results which he fancies would astonish systematists. Now in view of the amount of ink wasted over the questions of SPECIES, VARIETIES and CLASSIFICATION, it may be worth while to point out a few of the facts in the case; to show the futility of arguing on the abstract question of species, and incidentally to call attention to some of Mr. Wetherby's mistakes in dealing with certain varieties described by myself.

Mr. Wetherby says: "we may have species; we certainly have no subspecies." Now the truth is that *Nature* knows nothing of "species" or "subspecies," but only *individuals*. All *groups* of individuals are conventional and artificial. Were the record of palæontology complete, almost the entire mass of living individuals would be found to be connected throughout by intermediate forms. Not only would most of the *species* intergrade, but the genera, families

¹Land Shells of Roan Mt. and Vicinity, Journal Cin. Soc. N. H.

and orders likewise. I say "most" because some would not intergrade; for contrary to the old adage, nature *does* sometimes jump. The theory of descent, and the connection of distinct, recent species by their extinct ancestors being admitted, we may next inquire what convenient artificial limits may be erected to define the "species;" for all scientific investigation would be at a stand still if we have no names whereby to designate the various organisms about us. Probably the only definition of any use is that a species is any assemblage of similar individuals of presumably common ancestry, which cannot be connected by living intermediate specimens with other groups of individuals. It is the break in the chain which allows us to constitute the species; and whether this break be wide or narrow is of little importance so long as no recent organisms intermediate in characters are known.² However this idea may be worded, there is no possible foundation for species on any other basis. Now, many species, especially those having a wide range of distribution, show in some parts of their range considerable modifications usually correlated with peculiarities of climate, soil, or other factors known or unknown of their environment. These modifications are often sufficient for specific separation were it not for the fact that in some localities the links connecting the extreme forms occur. We have here species in process of making, waiting only for the extinction of the intermediate individuals or for the further intensification of the differential characters, to become full-fledged specific types. It is obvious that science must take cognizance of these incipient species, if it is to be a true record of nature; and for this reason "subspecies" or "varieties" are recognized. Of course they "run into" each other in some part of their range, otherwise they would be *species*. To ignore these varietal forms would be not removing obstructions from "the high-way of the Omnipotent" as Mr. Wetherby says, but a piece of the most pedantic falsity. The far-reaching importance of these local or geographic "subspecies" will be recognized when we understand that in them we have the material of future species in the making. We have moved away from the Darwinian conception that species have arisen from favorable variations of occasional individuals, preserved by the action of natural selection or "survival of the fittest"; and now we see much reason to believe that the whole mass of individuals over a given area of changed or changing conditions, is simultaneously remoulded, not

²The question of hybrids need not be considered here for obvious reasons.

by *individual variations* (which must usually be quickly effaced by interbreeding with normal or differently modified individuals), but by the steady action on the entire mass of the factors of climate, elevation, food-plants, currents and other quantities of the complex equation unknown to us.³

Mr. Wetherby proposes to avoid the use of subspecific or varietal names by the circuitous method of writing the *locality* after the specific name. He would say "*H. tridentata* Say, var. Campbell Co., Tenn." "*H. tridentata* var. Cincinnati, O." "*H. cereolus* var. Sanford, Fla." etc. Now the disadvantage of this system is that it tells absolutely nothing to the man who has no specimens from those exact localities, without a detailed description of the shells in each case. Moreover, Mr. Wetherby would write "*H. appressa* var. Woodville, Ala." for both *H. appressa perigrapta* and *H. sargentiana*, two very dissimilar forms. Who would know which one he meant to indicate? The trinomial system on the other hand offers a convenient, concise, readily understood index to geographic and local races. When one says "*P. cereolus septemvolva*" the idea is conveyed as exactly and much more concisely than by saying "*P. cereolus* large var. St. Augustine," for unless one has specimens from this locality he would not then know just what was meant. Again, were one to say "*P. cereolus* small var. Sanford, Fla." nobody could tell whether the variety found there was that with an internal lamina (*P. cereolus carpenteriana*) or without a lamina (*P. cereolus volvoxis*). Now what is the use in all this circumlocution when we have so convenient a system of nomenclature as the trinomial system, already in practical use in other departments of zoology.

Having discussed the abstract questions at issue at such length, we can devote but little space to the particular cases cited by Mr. Wetherby; but this is the less needful because what we wish to establish is the great importance of subspecies in general, not of any particular one of our own naming. We may, however, re-affirm the

³One of the most potent causes of specific or varietal differentiation has been the glacial epoch, which undoubtedly caused a southward movement of the entire northern fauna. Upon the recession of the ice sheet the species thus driven south found themselves exposed to changing climate and food-plants in their new home. Those following the retreat of the ice found the topography, soil and drainage systems of their former area in the north vastly changed. What wonder that we find many geographic subspecies! And shall we shut our eyes to the results upon our snails of the action of these cosmic forces, these manifestations of the Omnipotent?

reality of the distinction between the mass of southern, and the northern specimens of *P. appressa*. The northern shells described by Say have no incised spirals whatever, and the upper lip-tooth is frequently developed. The southern specimens (which we have called *P. appressa perigrapta*), have spiral incised lines more or less developed, and the upper tooth is wanting in the vast majority of cases. When it is present, as in the Cherokee Co., N. C. examples mentioned by Mr. Wetherby, I would regard it as an interesting case of reversion.

As to *Polygyra tridentata*, Mr. Wetherby has not read my paper with sufficient care to see my meaning. He attacks my *P. fraudulentata*, but says in the next sentence that *P. fallax* is perfectly distinct from *tridentata*. The truth is that *Helix fallax* of Wetherby and other modern authors and collectors is identical with my *fraudentata*! The true *H. fallax* of Say is identical with *H. introferens* Bland, as I have already stated in this journal and elsewhere. So my critic discredits and affirms the validity of this form in one article! As to *P. tridentata edentilabris*, Mr. Wetherby has evidently never seen it. The var. *juxtidentens* is a well-known form. I believe it to be a distinct line of differentiation, well worth attention and recognition by name.

I have not referred in this article to the large class of individual variations such as is shown in the banding of many *Helices*. This mode of variation is often repeated, different species having parallel modifications. The mutations are frequently not inherited, any of the forms giving birth to numerous others, as is the case with the band-varieties of *Helix nemoralis*. This tendency to "sport" in all directions is a totally different thing from the moulding of an entire race explained above; and its products cannot usefully be given varietal names. They are best expressed by formulæ devised to cover entire classes of such variations.

TYPES OF ANODONTA DEJECTA REDISCOVERED.

BY CHAS. T. SIMPSON.

In making a final arrangement of the general collection of *Unionidae* of the National Museum I found the other day among some